

SCIENCE

FRIDAY, DECEMBER 2, 1887.

A VERY SIGNIFICANT DISCUSSION on the subject of manual training took place at the late annual meeting of the school superintendents of New York State, held at Rochester. A year or two ago such a discussion would not have been possible. In the first place, the superintendents themselves would not have been able to discuss the subject intelligently at that time, nor would it have been regarded as at all a pressing matter. The events of the last twelve months have, however, conspired to bring about the result which made possible the discussion to which we refer. The continued agitation of the subject by those best qualified to discuss it, the increase of the intelligent literature on manual training, and the magnificent display of the results of this training which was made at the meeting of the National Educational Association at Chicago last July, have all had their effect. They have brought light to many minds where darkness was before, and produced a conviction even among the most determined scoffers at the movement. The discussion at Rochester was introduced by Superintendent Cole of Albany, in which city a very gratifying progress has been made toward the introduction of manual training, and whose school board has a most intelligent idea of the whole subject. The superintendents of Newburg, Dunkirk, Ogdensburg, Binghamton, Owego, and Elmira seem to have been to a greater or less extent in favor of manual training. The event of the discussion, however, must have been the remarks of State Superintendent Draper, for it was reserved for him to advocate manual training in the public schools, not because it is disciplinary, but because of its eventual utility. The attitude of the State superintendent only shows to what remarkable extremes the complete misunderstanding of this subject may be carried. We have frequently heard manual training opposed because of its utility, and because it was claimed that it has no disciplinary value; but Mr. Draper is the first person who has discussed the subject in public who has sufficiently misunderstood the whole subject to advocate it on that ground. He is reported as saying that he had no sympathy with the argument advanced, that industrial training should be carried on for its intellectual force. He claimed that the present school system of the State contained all the intellectual force that was needed. We fancy that the mere statement of these two propositions is sufficient comment upon them. It is hardly necessary to undertake to controvert them seriously. It would be interesting to know, however, whether Mr. Draper proposes to carry his theory into practice, and to eliminate from the school course all subjects which have a disciplinary value, and to replace them with those which have a practical utility. If so, the coming generation in New York may not know how to read, write, cipher, draw, and parse, but it certainly will be able to manage a steam-engine, lay transatlantic cables, and drive horse-cars.

THE DANGER TO COMMERCE from derelict vessels on the high seas cannot be too often pointed out, as it is not generally realized how long they are liable to keep afloat and pursue their aimless course, — a constant menace to navigation, and the cause, no doubt, of the loss of many a fine vessel by collision. This is well illustrated by the following instances, taken from the records of the Hydrographic Office, and it should be remembered that no such record can be complete. Long intervals often elapse without any report being made, and the track during this time, assumed as a straight line on the chart, must generally fall short of the actual

distance travelled. The ship 'Ada Iredale' (voyage from Androsan, Scotland, to San Francisco) was burned in the South Pacific through the spontaneous combustion of the coal with which she was laden. She was abandoned Oct. 15, 1876, latitude $13^{\circ} 30'$ south, longitude $107^{\circ} 45'$ west, about 1,900 miles east from the Marquesas Islands. The crew of twenty-three men reached the Marquesas group in twenty-five days, with the loss of one man and one of their three boats. The still burning wreck of the vessel drifted slowly to the westward in the south equatorial current, to Tahiti, Society Islands, 2,423 miles distant, and was towed into port by the French cruiser 'Seignelay,' June 9, 1877. She continued to burn till May, 1878, when she was repaired, and as a handsome iron bark, named 'Annie Johnston,' has done good service in the trade with China. The drift was 2,423 miles, and the time nearly eight months. The ship 'Oriflamme' was abandoned, on fire, in June, 1881, latitude $18^{\circ} 12'$ south, longitude $92^{\circ} 42'$ west. On Oct. 24 the steamship 'Iron Gate' (voyage from Adelaide, Australia, to Portland, Ore.) passed in latitude $13^{\circ} 27'$ south, longitude $125^{\circ} 19'$ west, an iron ship, apparently burned, with no masts standing, and sent a lifeboat alongside, but could see no signs of life. On Feb. 12, 1882, the hull of an iron ship laden with coal and iron drifted ashore on the island of Raroia, one of the Paumotu or Low Archipelago (latitude $15^{\circ} 55'$ south, longitude $142^{\circ} 12'$ west). She was visited by some natives, who brought away a small bell upon which was engraved "'Oriflamme,' 1865." She was completely burned out, and in a short time sank in deep water. The drift was 2,840 miles, and the time about eight months. The abandoned schooner 'Twenty-one Friends' was first reported March 24, 1885, about 160 miles off the capes of Chesapeake Bay, latitude $36^{\circ} 45'$ north, longitude $72^{\circ} 40'$ west. The Gulf Stream carried her in a direction about east-north-east, to latitude $51^{\circ} 30'$ north, longitude $27^{\circ} 40'$ west (2,130 miles in four months and a half). Thence she drifted in an easterly and south-easterly direction towards the northern coast of Spain, and was last reported Dec. 4 of the same year in latitude 45° north, longitude 8° west (about 130 miles north-north-east from Cape Finisterre). She was reported, in all, twenty-two times, which in itself shows how especially dangerous such a derelict is on the North Atlantic. The drift was 3,525 miles, and the time eight months and ten days.

A CONSPIRACY OF SILENCE.

THERE is an interesting discussion going on in England at present between Professor Huxley, Professor Bonney, and the Duke of Argyll. The question at issue is whether the influence of a great name has become so great in science as to interfere with free discussion in questions of a purely scientific nature. It seems that some seven or eight years ago Mr. Murray offered an explanation of the origin and structure of coral reefs which controverted some of the opinions expressed by Darwin. It is maintained by one side that this theory of Murray's has not been given free publication and discussion, and that, while it is intrinsically more probable than the older theory of Darwin, it is still held in obscurity by a conspiracy of silence on the part of the leading men of Great Britain. To make clear the present state of the controversy, we publish below the articles published in *Nature* by Professor Bonney and the Duke of Argyll.

[COMMUNICATION FROM PROFESSOR BONNEY.]

THE Duke of Argyll is eminent as a statesman, and has won distinction as a man of science. The mental qualities, however, which lead to success in these capacities are widely different; nay, in the opinion of some, are almost oppugnant. To the man of

science, truth is as a 'a pearl of great price,' to buy which he is ready to part with every thing previously obtained: to the statesman, success is the one thing needful, for the sake of which hardly any sacrifice appears too great. This is not said wholly as a reproach: it "takes all sorts to make a world." The ardor of the follower of the ideal, which may degenerate into recklessness, is wholesomely checked and beneficially qualified by the calmness of one who has to deal practically with mankind, and has learned by experience that evolution rather than catastrophic change is the law of life, and is in accordance with the analogy of nature. Still the two types of mind are commonly diverse, and the Duke of Argyll has recently afforded a remarkable instance of the extreme difficulty of combining in one person these apparently opposite characters.

This instance is afforded by an article which appeared in the *Nineteenth Century* for September last, and is commented on by Professor Huxley in the number for the present month. The duke's article bears the somewhat imposing title of 'The Great Lesson.' Professor Huxley's reply forms a part of an article entitled 'Science and the Bishops.' As the charge which the duke has in effect brought against men of science is a very grave one, and as some of the readers of *Nature* may not be constant readers of the chief monthly magazines, a brief notice of both accusation and reply may not be without interest.

The moral of 'The Great Lesson' is practically, 'Beware of idolatry.' The scientific world, in the duke's opinion, has been for some time bowing down to the idol of Darwin and the theory of evolution, which is the fundamental dogma of that cult. Like a prophet of old, he raises a warning voice, and points out that the feet of the golden image are in part composed of clay. In the North has been hewn the stone which shall shatter those fragile supports and lay the idol prone in the dust. To abandon metaphor, this is the state of the case. Among the results of Mr. Darwin's labors during the voyage of the 'Beagle' in the years 1831-36, when he accumulated that vast store of observations which served as a foundation for 'The Origin of Species by Means of Natural Selection,' was a theory of the formation of coral reefs and atolls, set forth in a volume entitled 'On the Structure and Distribution of Coral Reefs' (published in 1842 and republished in 1874). Of this theory the duke gives an outline in 'The Great Lesson,' executing this portion of his task so fully in the spirit of a just judge, and with so little of the craft of an advocate, as to leave nothing to be desired for lucidity of statement and cogency of reasoning. In fact, in the judge's summing-up, the case for the defence appears stronger than that for the prosecution; so much so, indeed, as to suggest that the difference is due to their inherent merits rather than to the mode of statement. However, be that as it may, the duke thus pronounces judgment, and in so doing passes a censure, stinging if deserved, on the men of science of this generation.

These are his words (*Nineteenth Century*, p. 305):—

"Mr. Murray's new explanation of the structure and origin of coral reefs and islands was communicated to the Royal Society of Edinburgh in 1880, and supported with such a weight of fact and such a close texture of reasoning, that no serious reply has ever been attempted. At the same time, the reluctance to admit such an error in the great idol of the scientific world, the necessity of suddenly disbelieving all that had been believed and repeated in every form for upwards of forty years, of cancelling what had been taught to the young of more than a whole generation, has led to a slow and sulky acquiescence, rather than to that joy which every true votary of science ought to feel in the discovery of a new truth, and—not less—in the exposure of a long-accepted error."

Again:—

"The overthrow of Darwin's speculation is only beginning to be known. It has been whispered for some time. The cherished dogma has been dropping very slowly out of sight. Can it be possible that Darwin was wrong? Must we indeed give up all that we have been accepting and teaching for more than a generation? Reluctantly, almost sulkily, and with a grudging silence so far as public discussion is concerned, the ugly possibility has been contemplated as too disagreeable to be much talked about; the evidence old and new has been weighed again and again, and the obviously inclining balance has been looked at askance many times.

But, despite all averted looks, I apprehend it has settled to its place forever, and Darwin's theory of the coral islands must be relegated to the category of the many hypotheses which have indeed helped science for a time, by promoting and provoking further research, but which in themselves have now finally kicked the beam."

This, then, is 'The Great Lesson':—

"It is that Darwin's theory is a dream. It is not only unsound but is in many respects the reverse of the truth. With all his conscientiousness, with all his caution, with all his powers of observation, Darwin in these matters fell into errors as profound as the abysses of the Pacific."

This is plain speaking. In words which admit of no ambiguity the duke declares that Darwin was wrong; that Mr. Murray set him right; and that the latter, instead of receiving a welcome, was met with a virtual conspiracy of silence on the part of scientific men. Of these three assertions,—which are to a considerable extent independent one of another,—the first and second are obviously very much matters of opinion, because, if the third statement be true, it is clear that no verdict has been delivered by experts, but that, like an Irish jury, they have professed themselves unable to agree, because the facts were so strong that even they could not bring in a verdict of acquittal. The third assertion, however, is much more a matter of fact, not difficult to substantiate, and at any rate, if incorrect, easy to disprove.

In regard, then, to the first and second, it may suffice to follow Professor Huxley's example, and be content with expressing a doubt as to the accuracy of the duke's assertions. In the face of statements so definite as those quoted above, this may seem presumptuous. They read almost like the sentence of an ecclesiastical court, which it is heresy to question. *Caledonia locuta est, causa finita est*, seems to be their tone; and if one whisper a doubt, one expects the familiar conclusion, *Anathema sit!* But men of science, as all the world knows, are sceptics. Have they yet awakened and rubbed their eyes, and said of Darwin's theory, "Lo! it was a dream"? What says Professor Huxley? He asserts that Darwin's confidence in the accuracy of his own theory was not seriously shaken, as the duke alleges, and quotes as conclusive evidence a letter from Professor Judd, who gives the results of a conversation which he had with Darwin no long time before the death of the latter. Professor Huxley also intimates that to himself, though tolerably familiar with coral reefs, the new theory is at first sight so far from fascinating, that, until he can devote a considerable time to a re-examination of the whole subject, he must be content to remain "in a condition of suspended judgment," and that Professor Dana, "an authority of the first rank on such subjects," has pronounced against the new hypothesis in explicit terms. Undoubtedly, Mr. Murray has obtained distinguished converts, but with such differences of opinion among those best qualified to judge, it is certainly going further than is warranted by facts to insinuate, if not to assert, that he has convinced the scientific public. Very probably more than a minority of them are in my own position, which perhaps I may be pardoned for stating. They, like myself, have never had the opportunity of forming an independent judgment upon the matter, but they see some very serious difficulties—difficulties which are of a general rather than of a special nature—in the new explanation. At present these difficulties do not appear to them to have been overcome; so that, while admitting that Mr. Murray's hypothesis may sometimes apply, and that Darwin either may have expressed himself a little too sweepingly, or may have been understood so to do, the theory of the latter is capable of a more general application, and presents less serious general difficulties, than does that of Mr. Murray.

We come, then, to the third charge, which is the most serious one, because it affects the morality of scientific men; and many of them, like myself, are old-fashioned enough to resent being called a knave more than being called a fool. Has Mr. Murray been met by 'a conspiracy of silence'? The duke, in asserting this, must have been strangely oblivious of, or, among the cares of a statesman, have failed to keep himself *au courant* with, the literature of geology. Professor Huxley denies the assertion, and adduces in his support an answer to an inquiry which he had addressed to Professor Judd. The facts according to these authorities are briefly as follows: Mr. Murray's views were duly published, as the

duke himself states; they were favorably regarded by the authorities at the 'Challenger' office; they were expounded, one might almost say advocated, on more than one occasion by Dr. A. Geikie. His text-book in the year 1882 not only took the leading place, as it still does, but also was then the only complete text-book on a large scale for this country. On p. 468 is a full statement of Mr. Murray's views. They have also been referred to at more or less length in many treatises and journals, both English and foreign. As Professor Judd remarks, "if this be a 'conspiracy of silence,' where, alas! can the geological speculator seek for fame?"

Thus the main charge is disproved. One special item in it, however, as peculiarly offensive, yet calls for a brief notice. The duke states, "Mr. John Murray was strongly advised against the publication of his views in derogation of Darwin's long-accepted theory of the coral islands, and was actually induced to delay for two years." Now, if these words do not amount to an imputation of bad faith on the part of Mr. Murray's adviser, and are not by insinuation extended to others, I do not know what they mean, or why they have been penned. But, as Professor Huxley observes, "whether such advice were wise or foolish, just or immoral, depends entirely on the motive of the person who gave it." The remark is perfectly just. Who, I would ask, who is old enough to look back on a quarter of a century of work, has not occasionally said, "Wait a bit," to some younger friend, who has come in the first incandescence of a brilliant hypothesis? I have so sinned. Sometimes I have been wrong and my young friend right, but not always. Still, I know myself fallible. As the late master of Trinity said, "We are all fallible mortals, even the youngest amongst us." Yet I am not ashamed. I will not put on sackcloth and ashes, and I mean to sin again. Perhaps it is because I am naturally unimaginative; perhaps I am come to the season of autumn leaves; but I have always looked askance at a brilliant hypothesis, and now distrust it more than ever. I have lived long enough to see many a one go up *whoosh!* like a sky-rocket, all stars and sparks, and come down exploded, all stick and stink.

So the 'great lesson' has been read, and the scientific world, I fear, has not repented or rent its clothes. But it has heard, and not without indignation. The Duke of Argyll has made grave charges against the honor and good faith of men of science, and they ought to be grateful to Professor Huxley for his prompt repulse of the attack and his stern rebuke of the assailant. As it seems to me, reply is only possible on one point; namely, the special charge mentioned above. Hence the Duke of Argyll is bound to establish or to withdraw the accusation.

Men of science are justly sensitive on this question. Doubtless they are no more exempt from human frailty than any other class of men: we all fail sometimes—nay, too often—to live up to our ideal standard; still, such shortcomings are not common, and any thing like a 'conspiracy of silence' or any kind of scientific 'boycotting' is a thing so improbable as to be almost incredible. Each man must testify according to his own experience: so in conclusion, though it may be deemed impertinent, I will express my own. I have lived now for not a few years among the rank and file of scientific men on more intimate terms than can have been possible for the Duke of Argyll, owing to his exalted station and his high occupations of state, and I am bound to declare, that, in a fairly wide experience, I have never found men as a class less self-seeking or more earnest in their desire for truth, more steadfast as friends, or more generous as antagonists.

T. G. BONNEY.

[COMMUNICATION FROM THE DUKE OF ARGYLL.]

THE article which I contributed to the September number of the *Nineteenth Century*, on the coral islands of the Pacific, has done what I intended it to do. It has called wide attention to the influence of mere authority in establishing erroneous theories and in retarding the progress of scientific truth. The vehement assault made upon it in the current number of the same review by Professor Huxley, and the article by Professor Bonney in this journal, are to me gratifying evidences of success. But both of these writers are entirely wrong in the interpretation they put on a few expressions in my paper. They interpret these expressions as conveying imputations on the probity and honor of scientific men in the habitual and wilful suppression or discouragement of what they know

to be truth. But there is nothing to justify this interpretation. I have made no such accusation, and, if any one else were to make it, I should join the two indignant professors in repudiating it. Scientific men are not only as good as other men in this way, but generally a great deal better. Professor Huxley has been irritated by some 'anonymous sermon,' which I have not seen, and for which I am not responsible. He admits that it is in this anonymous production that the 'slanders' against scientific men have taken the peculiarly offensive form; but he maintains that this unknown writer has been 'inspired' by my article on coral islands. On the strength of this assumption,—which may be true, for aught I know,—he goes on through some seven pages to dissect certain parts of my paper, and to read into it a great deal that is due to his own excitement, and to nothing else.

I have no difficulty in expressing clearly, and without any circumlocution, exactly what I do mean, and what I have intended to say. Professor Bonney interprets it very fairly, in abstract, when he says that the moral of my paper is, 'Beware of idolatry.' Some theory, hypothesis, or doctrine is propounded by a great man. It becomes established, partly perhaps by certain inherent elements of strength, or, at all events, of attractiveness. But soon it stands unassailable and unassailed upon the vast foundations of general acceptance and admitted authority. It becomes what Professor Huxley on a celebrated occasion, and with at least a momentary insight, called 'a creed.' The effect of such a position is tremendous. Some men who see cause to doubt are daunted. They keep silence. Others are prevented from even thinking on the subject. A few who do think, and who do doubt, and who do venture to express their doubts, are discouraged and discountenanced. A great many others take refuge in a suspended judgment, even after the production of evidence, which, in the absence of a 'creed' and of authority, would have been deemed conclusive. In all this there may be, and in general there is, nothing worse than timidity on the part of those who are the laggards, or the opponents, in some great advance. It is more difficult for some men than for others to face a prevalent opinion or an accepted doctrine. It is all very well to say, as Professor Bonney says, that "to the man of science truth is a pearl of great price, to buy which he is ready to part with every thing previously obtained." But scientific men are human. They are, I admit, immensely superior to the politicians, especially just now. But they have their failings; and every one who knows the history of science must be able to call to mind not one instance only, but many instances, in which the progress of knowledge has been delayed for long periods of time by the powerful and repressive influences of authority, exerted in one or other of many ways.

My contention is, that Darwin's theory on the origin of the coral islands is a case in point. I believed in it, or accepted it, for many years, as others did. Professor Bonney admits that I have described it not only fairly, but as forcibly as if I were still its advocate. This is exactly what I tried to do. I now hold that it has been disproved, and has been replaced by another theory quite as grand, and more in harmony with other natural laws which are of universal operation, but have been only lately recognized. I affirm, further, that this new theory or explanation has been received with the timidity, the discouragement, the discountenance, and the obstruction which are characteristic in such cases. That Dr. Geikie has supported it, is most creditable to him. But his voice is not enough to disprove the truth of my contention. That Professor Huxley and Professor Bonney should be unable to make up their minds upon such evidence as has been before us now for several years, is, in my opinion, a strong confirmation of the law which is operating upon them. There are some discoveries in science—some explanations of curious phenomena—which are self-luminous. They shine with their own light. The moment they are suggested, with a few cardinal and certain facts to illustrate them, they are their own proof. Every thing that turns up speaks in support of them. My conviction is that such is the character of Mr. Murray's theory of the coral-island formations in the Pacific.

Professor Huxley challenges me to re-affirm with better proof the fact I allege,—that Mr. Murray has met with discouragement. I respond at once to that challenge. I have seen the letter from Sir Wyville Thomson in which that naturalist urged and almost insisted that Mr. Murray should withdraw the reading of his papers

on the subject from the Royal Society of Edinburgh. This was intaker or somebody informed me." Then the culprit is brought before him, and the fact revealed that he indirectly aided a criminal. February, 1877. No special reason was assigned, but the terms of the letter indicate clearly that Sir Wyville dreaded some injury to the scientific reputation of the body of naturalists of whom he was the chief, and for whom, as connected with the 'Challenger' expedition, he was in some degree responsible. He had not himself at that time, I believe, fully accepted the new doctrine. But that would have been no sufficient reason for discouraging free discussion, if it were indeed as free as it ought to be. In my article I understated the delay which was thus occasioned. Three years, not two, elapsed before Mr. Murray was at perfect liberty to advocate his views in the proper place, before a scientific body.

But the challenge of Professor Huxley has brought to my knowledge a new bit of circumstantial evidence to the same effect, which is highly significant. Among the investigators of the Pacific corals, no man has done better work than Dr. Guppy, surgeon of H.M.S. 'Lark.' Since my article was written, his volumes on the Solomon Group of islands have been published. The geological volume is an admirable memoir. It is the record of observations as patient, detailed, and conscientious as have ever been made on the great geological problem which is at issue. After his return home, he was advised by Mr. Murray to offer a paper on his researches to the Geological Society of London. He did so in the spring of 1885. But his paper was refused, much to Dr. Guppy's disappointment. It was not orthodox. His facts effectually removed some difficulties in the way of Mr. Murray's theory, — facts which in more than a corresponding degree were adverse to the theory of Darwin. As a consequence the Royal Society of Edinburgh has had the honor of receiving and publishing Dr. Guppy's most interesting memoir. As a Scotchman, I am proud of this contrast. I make no accusation of wilful unfairness against the authorities of the Geological Society of London, of which my critic Professor Bonney was, I believe, at that time the president. They did not consciously discourage truth. On the contrary, they probably smelt heresy. But if their minds had been free from this prepossession, — if they had been alive to the breadth and sweep of the questions at issue, and open to receive with welcome the crucial evidence bearing upon them which is contained in Dr. Guppy's paper, — the rejection of it would have been impossible.

As regards Darwin's own state of mind upon the subject, I can only say that my information was as good as that in the possession of Professor Huxley. I am not struck by the perfect candor of his reference to Darwin's letter to Professor Semper in October, 1879. If he had quoted the very next sentence to that which he does quote, a very different impression would have been left on the reader's mind. But I attach no importance to this point. I prefer to believe that Darwin's mind was open to conviction, and to hope that others will follow his example.

ARGYLL.

THE AMERICAN PUBLIC HEALTH ASSOCIATION.¹

DR. CARL HORSCH of Dover, N.H., read a paper entitled 'The Necessity of Burial-Permits and Inspection of Bodies of Deceased Persons.' He based this necessity on the following grounds. (1) It is the best safeguard against the possibility of premature burial, and also that the apparently dead may not be placed in cold rooms or on ice, and frozen to death. (2) Cases of concealed contagious and infectious diseases will be detected, and an epidemic may be averted. (3) Murder and suicide may be detected; and if cremation, the surest method for the destruction of disease-germs, is generally established, there will be also less danger that the body of a murdered person will be cremated, and the crime concealed. (4) Life-insurance frauds may be prevented. (5) Where the fear exists of being buried alive, the family physician can overcome that fear by that examination, and his assurance that the loved one is dead. (6) In order to sign a certificate for a burial-permit legally, that inspection gives the most important evidence. If a physician gives his signature to such a certificate without seeing the body, he may be brought in the following unpleasant position: he is called into court, the certificate is laid before him, the questions asked, "Did you sign that certificate?" Answer, "Yes." "Did you know that the person was dead?" The only answer could be, "The under-

taker or somebody informed me." Then the culprit is brought before him, and the fact revealed that he indirectly aided a criminal.

Dr. Rohé, secretary of the committee on disinfectants, presented the report of that committee. The following conclusions were drawn from their work: —

1. The temperature required to destroy the vitality of pathogenic organisms varies with the different organisms.

2. In the absence of spores the limits of variation are about 10° C. (18° F.)

3. A temperature of 56° C. (132.8° F.) is fatal to the bacillus of anthrax, the bacillus of typhoid-fever, the bacillus of glanders, the spirillum of Asiatic cholera, the erysipelas coccus, the virus of vaccinia, of rinderpest, of sheep-pox, and probably of several other infectious diseases.

4. A temperature of 62° C. (143.6° F.) is fatal to all of the pathogenic and non-pathogenic organisms tested, in the absence of spores (with the single exception of *sarcina lutea*, which in one experiment grew after exposure to this temperature).

5. A temperature of 100° C. (212° F.), maintained for five minutes, destroys the spores of all pathogenic organisms which have been tested.

6. It is probable that some of the bacilli which are destroyed by a temperature of 60° C. form endogenous spores, which are also destroyed at this temperature.

Dr. John S. Billings, U.S.A., read a paper on some forms of tables of vital statistics, with special reference to the needs of the health-officer.

A resolution was adopted to appoint a committee, with Dr. Sternberg as its chairman, to study experimentally the methods and effects of protective inoculation against infectious diseases.

Dr. Horsch presented a paper entitled 'Inspection of Animals required for Food,' in which he recommended the inspection of animals by competent persons before they are slaughtered, and a thorough examination of their viscera afterwards.

Dr. Azel Ames, jun., of Chicago presented a paper on the meat-food supply of the nation, and its future. In it he gave statistics of the resources of the country with reference to its animal food, and showed, that, as the population increased and the grazing country diminished, these resources were proportionately declining. He criticised adversely the policy of the general government in dealing with the public lands. Legislation was asked of Congress for the suppression of pleuro-pneumonia, and for the taking of a thorough census of the cattle of the country and their products. Dr. Ames denounced the tax imposed by the oleomargarine act as being unjust to the poor, and wrong in principle, and demanded its repeal.

A paper by Dr. J. H. Rauch, secretary of the State Board of Health of Illinois, dealing with the subject of cholera and quarantine, excited great interest. Dr. Rauch described the defects of the quarantine at the port of New York, and said that in the West its results were looked upon with distrust. He asked that the entire quarantine system of the United States should be placed under national control. In the discussion which followed, Dr. A. N. Bell criticised most severely the arrangements of the New York quarantine, but expressed the opinion that the measures which had been applied by the health-officer in the management of the passengers of the steamers 'Alesia' and 'Britannia' had been successful.

The paper of Dr. Dickinson was discussed by a number of the members of the society. Dr. Eliza M. Mosher remarked that the point of greatest interest in connection with the subject was whether the loose corset injured the health of the wearer, and, if so, what could be offered as a substitute. Most girls, according to her experience, wore them sufficiently tight to limit respiratory movements. It was difficult to measure the injury done, since the chest was already crippled, and its expansion was below its possibilities. In addition to the thinning of the abdominal wall described by Dr. Dickinson, there was atrophy of the entire surface covered by the corset, with lack of development of muscular tissues due to restricted movement. This was apparent by the often-repeated remark of ladies that they could not sit up straight without their corsets. It was often observed how useless were the arms of most young ladies for any manual labor, even though their lower extremities were capable of long-continued muscular movement. A well-developed nipple was almost an unknown thing with a woman or girl who had worn

¹ Continued from *Science* of Nov. 25.

a corset for any length of time. The respiratory murmur below the fifth rib was very faint compared with the sounds above, and these ladies found it impossible, as a rule, to move the strength 'spirometer' the fraction of a degree. From these facts, she concluded that the capacity of the chest had become limited, and the muscular fibre of the diaphragm impaired, by the unyielding walls of the corset-prison. Not very great compression upon the line of attachment of the diaphragm was required to interfere with its contractile power. Loss of strength in the abdominal muscles and diaphragm prepared the way for a slow and painful, if not instrumental, labor. Loss of respiratory capacity implied increase in rapidity of the heart's movements: this meant weakening of its force, and thus came the cold extremities and easily chilled skin so common in those who wore corsets. She had not been able to demonstrate the displacement of the liver spoken of, because, doubtless the examinations were made with the corset off. With a large experience in treating girls suffering from displacements of the uterus (mainly retroversion and downward crowding), little could be done to relieve the sufferers until the corset was laid aside. Active muscular movements and corsets were not compatible, and unless the corset and its equivalent, tight clothing, were discarded, she was not sure but girls were better off without active physical exercise. What could we substitute for the corset, which, without producing pressure or displacement, would give the trim and tidy look so much admired by the sterner sex? A good dressmaker had more to do with this matter than the corset had. An underwaist without bones, with skirts snugly fastened to it; a dress-waist well shaped, containing a few bones, and loose enough to permit a long breath without limitation, — would make nine girls out of ten look just as trim and tidy as a corseted waist. If something more supporting was demanded, the 'Ferris waist' was all that was required. Without steels in front, and without bones, if worn loosely with the skirts attached, it might be accounted healthful. Dr. R. G. Eccles spoke of the comfort women derived from the corset. He had noticed that the more intelligent the women, the more they were corset-wearers. Dr. Jerome Walker spoke of the evils of corset-wearing, among which he mentioned the shallow breathing as particularly objectionable. Dr. William Anderson thought if we educated the women to despise the corset, it would disappear. The president remarked that we did not use our chests to their full capacity except when making unusual exertion. A woman under ordinary circumstances had her breathing but little restricted. If the servant laced herself in her ordinary working costume as she did on Sunday, she would suffer severely.

MENTAL SCIENCE.

The Mechanism of Attention.

M. TH. RIBOT, whose useful compilations on English and German psychological movements, on heredity, on the diseases of memory, of will, and of personality, have gained for him a world-wide reputation, contributes to the *Revue Philosophique* (of which he is the editor) an interesting and convenient account of the mechanism of the processes of attention.

Attention is not so simple a phenomenon as popular analysis makes it. It is not always one and the same state, but varies indefinitely in intensity, from the momentary attention necessary to brush off a fly, to the most complete absorption. This intensity is gained either by the accumulative results of a long-continued strain, or by an intense focusing of all energies to one group of sensations. Regarding attention thus as a state varying in degree, we are ready to make a distinction on which M. Ribot lays much stress; namely, between spontaneous and voluntary attention. Our notion of attention is derived almost exclusively from the latter. Attention we regard as a purposive effort. But this is really not the typical nor the most important aspect of attention. The former has been much neglected, and to it M. Ribot devotes his first article. The distinction between the two forms of attention is easily made clear. The first is a natural impulse to let such things make an impression upon us as excite our interest. The second is an artificial product of civilization, that we have learned at school. To look at flowers and be impressed by them is a result of spontaneous attention; to

dissect and minutely analyze their parts, of voluntary attention. The main characteristic of attention, and especially of its typical, natural form, now under discussion, is its motor aspect. As Maudsley puts it, whoever is incapable of controlling his muscles is incapable of controlling his attention. All attention, however, is, in a sense, an abnormal, exceptional state. Such states cannot last long, because they are opposed to the ever-present change that is the law of life. We see this abnormality when attention is carried to the extreme, producing clouding of the mind, a mental void, or vertigo. Their analogy with fixed ideas and states of ecstasy is also close. The normal process of 'cerebrising' consists of an ever-changing focusing on one set of impressions, then a diffusion of these to give place to another group, and so on in an ever-successive lighting and skipping; the laws of association governing the order and connection of the several foci. Normal thought is thus a 'poly-ideism,' while attention is a 'mono-ideism.' It is a focus concentrating into itself all the available energies; it is the substitution of unity for diversity. Attention is further characterized by being directed towards an end: it is not a subjective process, but is adaptive, convergent. If a definition be desirable, we might define attention as "an intellectual mono-ideism with spontaneous or artificial adaptation of the individual."

Spontaneous attention is well seen in children and in the higher animals. Its cause is universally an *emotional* state. It is only the sensation-exciting, the interesting, the agreeable or disagreeable, that is naturally attended to. An animal incapable of feeling pleasure and pain would by that fact be incapable of attention. This general fact is exemplified in the biographies of great men, showing in some cases how the hero of the tale is for a long while restless, listless, until he falls upon the occupation that interests him, enthuses him, and brings out his genius by focusing his attention to a single line of thought. This passion for work has its analogue in other less desirable passions. The drunkard's attention is critically intense in the presence of the glass. But these intense states cannot endure long, and they only last as long as they do because a small amount of flitting really goes on, continuous as the state seems to us.

The physical conditions and accompaniments of attention are of great importance. The general law under which they are to be considered is that there is no thought without a tendency to its expression in motor terms. Thought is initial action. The motor expressions of attention are visible in three directions, — the vaso-motor phenomena, the respiratory phenomena, and the expressional phenomena. The first is recognized in the increase of blood in certain parts of the brain under mental work, as ascertained by direct experiments of Mosso and others. The slightest mental strain produces this result. The second is characteristic of the attitude of attention. The breath is slowed or held; sighs occur; and all this suggests the abnormality of the process. The third kind of movements are psychologically the most interesting; and many theories, notably that of Darwin, have been proposed to explain their origin. Duchenne experimented by applying electrical stimuli to muscles of etherized patients, and noting the facial expression thus aroused. He regarded the contraction of a single muscle as characteristic of one emotion. The frontal muscle furrowing the forehead is the muscle of attention; the orbiculars contracting the orbital space and lowering the eyelid of reflection, and so on. The motor expressions will be different according as the attention is directed inwards (reflection, contemplation) or outwards, as is usually the case. The motor expressions of the two are opposed: in the one the forehead is lowered, in the other the eyelid is lowered, the mouth closed as in effort, and so on. Darwin calls the attitude of reflection that of difficult vision turned inwards. The general attitude of attention is immobility, a tendency to unity of action, to convergence. It is a concentration of both motions and thoughts; and the degree of attention is inversely to the amount of motion. An attentive audience is quiet: an inattentive one shuffles and moves in a hundred ways.

To this rule there is an apparent exception in the common habit of walking, beating, etc., when deep in thought. This is to be accounted for by the increase of brain-activity thus brought about. Such movements are dynamogenic, re-enforcing, arousing the motor centres, and thus adding to the available energy.

The accompanying motions of attention are not merely signs of the former, but are essential, constituent factors of it. Suppress the expressive movements, and you suppress the whole process. The fundamental rôle of these movements is to keep up and reinforce the attentive consciousness. The brain in attention acts both as an intellectual and as a motor organ.

A special form of spontaneous attention is surprise, and it is simply an exaggerated form of it. Descartes has given a good account of it, recognizing (though in other terms) the increase of nervous influxes that accompany it, the direction of energy towards muscles, and the typical facial expression. Surprise is a shock caused by the unexpected, a sort of emotional hiatus. This lasts until the object that caused the surprise is apperceived, recognized, adapted to. In surprise one feels much, and knows little; and the intense emotion rivets the attention. On the physical side the symptoms are exaggerated, the eyelids are widely opened,¹ perhaps the mouth too.

The utility of attention in the struggle for life is evident. The moment differentiation is clear, one part of the organism concentrates the energy and arouses a rudimentary attention. Riccardi places the origin of attention in the arthropods. The attention gets centred upon the most perfect sense in the animal, whichever that is. In the higher animals attention is marked, and in all such as play, showing thereby a surplus of energy, there is also an attention to objects not directly useful in the struggle for existence. This is the higher form of attention, equally evident in children.

In a succeeding article, Professor Ribot will give a similar exposition of voluntary attention.

PRIMITIVE MIND. — An interesting glimpse into the thought processes of unenlightened peoples is furnished by the following observation of the Ainos (a degenerate Japanese tribe distinguished for their long growth of hair) during the recent eclipse. The Aino is said not to be imaginative, but, on being shown the eclipse through a smoked glass, he cried out that the sun was fainting away and dying. A silence ensued, broken by an exclamation of fear that the sun would dry up. They brought water and sprinkled it upwards towards the sun, crying, "O god, we revive thee! O god, we revive thee!" Some squirted the water upwards with their mouths, some threw it up with their hands. A group of women and girls sat down with their heads between their knees, as if expecting some calamity. Their tradition with regard to the eclipse says that "when my father was a child, he heard his old grandfather say that his grandfather saw a total eclipse of the sun. The earth became quite dark, and shadows could not be seen; the birds went to roost, and the dogs began to howl. The black, dead sun shot out tongues of fire and lightning from its sides, and the stars shone brightly. Then the sun began to return to life, and the faces of the people wore an aspect of death; and, as the sun gradually came to life, these men began to live again." Otherwise they have no theory of the eclipse, but their personification of the phenomenon is evident.

EXPLORATION AND TRAVEL.

Wissmann's Expedition across Africa.

LIEUTENANT WISSMANN, whose journeys in the Kongo basin won him a well deserved fame as a traveller and energetic explorer, has just returned from his second expedition across the African continent. In the spring of 1886 he started from Angola for Luluaburg, a station of the Kongo Association which is situated in the empire of the Muata Yambo. Since Pogge's first journey in 1876, there have been six Muata Yambos, and, as at the death of the ruler the capital is changed, six capitals of the empire. We gave a report of Wissmann's expedition from Luluaburg to the Baluba country in No. 228 of *Science*. On his return from this excursion, he found one building of the station, which contained twenty-one rooms, burnt down, and the commander sick with malaria. His description of the station is of some interest. It consists of a number of houses for the officers of the station, barracks, a house for twelve women, stables, and a prison. The latter is called the cold house, as it is not permitted to have a fire in it during the

night, — a regulation which is much feared by the negroes, the nights being very cool on the high plateau. The station is protected by a stockade, and a *glacis* three hundred feet in width. The roads in the neighborhood of the station are fifteen feet wide, and kept very clean. About two thousand feet from the station a village of the Bassilange is situated.

Wissmann's expedition, when starting from Luluaburg in October, 1886, consisted of eighty-nine persons, among whom were an interpreter, a Zanzibari, and thirteen Angola men, while the rest were Bassilange. The number of people, however, rapidly increased to about one thousand, as the Lukugesha, the empress of the Muata Yambo Empire, and the son of Kalamba, with their followers, joined the expedition. When they arrived on the Lubi, an excursion into the country of the Benangongo, who live on the right bank of the river, was made. Then the river was followed to its confluence with the Sankuru, which was crossed below the mouth of the Lubi. It was originally Wissmann's intention to explore the country north-east of this river, which forms the watershed between the Sankuru and the Kongo. He found, however, the state of affairs in the country east of the Sankuru so much changed since the time when he visited it first, four years ago, that he was unable to carry out his purpose. While formerly the cowry was the principal object of barter, now guns and ammunition were in demand. The slave-trade is flourishing. The chiefs of the Bassonge and Bassenge, frequently supported by slave-traders, make raids upon the neighboring tribes in order to procure slaves. These are bartered to the traders for guns and ammunition, or for ivory to the Bakuba, who buy the women for their household, the men for being sacrificed at burials. A short time before Wissmann's arrival a chief of the Bakuba had died, and two hundred slaves were killed when he was buried. Travelling eastward, Wissmann crossed a vast belt of primeval forest which is inhabited by Batetela and the dwarfish Watwa. The woods are almost void of large animals, and even birds are scarce. On the Lukassi the expedition was attacked by the natives, who killed several persons with their poisoned arrows. But after a lively skirmish the natives were driven off, and, when the expedition reached their villages, they were found deserted. During the month of January, 1887, Wissmann crossed a territory depopulated by war and small-pox. The country of the industrious Beneki, whom he visited on his first journey, he found entirely devastated. Famine and small-pox prevailed among the members of the expedition, and it was not until the Lomami was crossed that matters became more favorable. At last Nyangwe was reached. Wissmann found the Arabs of this place in a state of great excitement on account of the events at Stanley Falls. Nevertheless he succeeded in returning the Bassilange to their native country, but Wissmann himself had to give up the hope of further explorations, and proceeded on the well-known route to the Tanganyika and by way of Lake Nyassa to Zanzibar, whence he returned to Europe.

The results of this expedition are not so important as it was hoped they would be when Wissmann started from Luluaburg. An expedition from the Kongo southward, or from Luluaburg north-eastward, is what is wanted to give us a more thorough knowledge of the hydrography of Central Africa.

THE HUDSON BAY EXPEDITION OF 1886. — Lieutenant Gordon's report of the last expedition of the 'Alert' to Hudson Bay makes it clear that all hopes of establishing a trading-route from England to the west coast of Hudson Bay must be abandoned. The navigation of Hudson Strait proved extremely dangerous on account of the prevailing fogs, the strong tides, and the narrowness of the waters, but principally on account of the heavy ice of Fox Basin, which frequently obstructs the western entrance of the Strait, and of the faulty working of the compass. Besides, vessels navigating these waters must be fortified for meeting the ice, and must not be larger than two thousand tons, because a larger ship would be somewhat unwieldy, could not make such good way through the loose ice, and, being unable to turn so sharply, she would get many a heavy blow, that a smaller ship would escape. Gordon supposes that navigation can be opened between the 1st and 10th of July, and that the closing of the season would be about the first week of October. These results of Lieutenant Gordon's experience agree exactly with what was maintained by all experts when the scheme was first propounded; but at that time their

¹ That this is instinctive is borne out by the fact that it occurs in those blind from birth, and in whom opening the eyes could not thus increase sensation.

views were disregarded in Canada. Although the principal object of the expedition has failed, its scientific results are considerable. These consist chiefly of the meteorological and hydrographical observations of two years, from the fall of 1884 to the fall of 1886, and other occasional remarks of the observers. The results of these observations are laid down in a meteorological atlas of Hudson Bay, but it seems to us that the available material is too scanty for constructing the monthly isothermal lines over so vast a territory. The report is accompanied by a plan of Churchill Harbor and York Roads (at the mouth of Nelson River), from the surveys of Lieutenant Gordon. The general track-chart is not very elaborate, and in many parts not up to date. Several changes in the coast-line appear, for which no evidence is given; e.g., the division of the main island of Southampton into two parts. The publication of several charts and plans based on surveys of the expedition is promised at the end of the report.

ETHNOLOGY.

The Eskimo Tribes.

DR. RINK, who has for a long time maintained the American origin of the Eskimo, has published the results of his long-continued observations and studies in the eleventh volume of the *Meddelser an Grönland*. Fortunately the volume, the publication of which has long been wished for by all students of Arctic America, is written in English, and thus made accessible to a wide circle of readers. Rink has propounded his views on the origin of the Eskimo in several papers, which were published in various journals. He believes that they descended from the interior of Alaska to the coast of the Arctic Ocean, and gradually spread eastward. His arguments, which form the first part of his book, are based on a comparison of the implements, dress and ornaments, domestic industry and arts, religion and folk-lore, and sociology of the Eskimo of the various parts of Arctic America. The results of this investigation are, that the hunting-implements are the more highly developed the farther we proceed eastward, that the style of dress and habitations show a gradual approach to the Greenland style from west to east, and that the western tribes occupy a higher stage of social organization than the eastern ones. Among the customs which prevail among the western tribes, but gradually disappear eastward, he mentions the use of the labret, and the religious festivals in which masks are used.

Conclusions drawn from these facts are necessarily open to discussion, as these phenomena may be explained in different ways. I think attention should be called to the fact that all the peculiarities of the western tribes may be derived from an influence of the North-west American culture. We have the extensive use of masks, the peculiar wooden hat of the southern Eskimo tribes, the use of the labret, the festivals in which property is given away, the houses built on the same plan as Indian houses, the sweat-bath, the existence of slavery, and the high development of the art of carving. The existence of so many similar or identical phenomena in two neighboring nations cannot be fortuitous. Besides, I have to mention that the folk-lore of the tribes of British Columbia refers to the Eskimo country and to the Eskimo as plainly as possible. The legends of tribes of Vancouver Island speak of a country in the far west, where the sea is always covered with ice, where the nights are very long, and where people live who use skin boats. Considering the great uniformity of Eskimo life all over Arctic America, I cannot but conclude that here an immediate influence of the North-west Americans upon the Eskimo had place, and that west of the Mackenzie we do not find the latter in their primitive state of culture. It is not impossible, that, in consequence of this influence, inventions and customs which were originally Eskimo (as the kayak) became more neglected than they are in other regions where foreign influences were not so strong.

But we have to consider several other points. The use of masks representing mythical beings, which is peculiar to North-west American tribes, is not entirely wanting in the east. The giving-away of property at certain festivals, and the use of the singing-house, with a central fire and places for the people all around the wall, may also be traced as far as Davis Strait. It may even be that the plan of the stone or snow house of the central Eskimo,

with elevated platforms on three sides of a central floor, must be traced back to a square house similar to that of the western tribes.

I will not enter into a discussion of the similarity between Eskimo and Indian folk-lore, as we are not sufficiently informed about this subject. The few traces which are common to both are so wide-spread that they cannot be considered proof of an early connection between these nations. The story of the dog who was the ancestor of certain tribes, the transformation of chips of wood into salmon, the idea that animals are men clothed in the skins of animals, stories of children who were deserted by their relatives and became rich and powerful by the help of spirits, are common to the folk-lore of North-west America and the Eskimo.

It seems that the only safe conclusions one can arrive at are the following. The Eskimo reached an ice-covered ocean as one body. At that time their religious ideas and implements were similar to what we observe at the present time. They knew the kayak and the sledge, they lived probably in large square houses, they had domesticated the dog, and it is not improbable that they had certain festivals which referred to the seasons or to the sun. Besides this, we are inclined to suppose that they were fishermen, and were accustomed to the use of boats before they came to the Arctic Sea. These conclusions seem to point out that the Eskimo spread from the great rivers of central Arctic America.

In order to make satisfactory progress in the puzzling problem of the origin of the Eskimo, the influence of the North-west Americans upon their Arctic neighbors, and the origin of the folk-lore of the Tinne and western Eskimo, must be studied. In our present state of knowledge, we can consider the American origin of the Eskimo only a theory, which is more probable than an immigration from Asia.

The principal part of Rink's book is an excellent treatise on the Eskimo grammar, and a comparative list of the independent stems of the Eskimo dialects. The stems are arranged in alphabetic order, and to each is added the dialect in which it occurs. As the Greenland dialect is by far the best known, it is made the basis of the list, and all other dialects are referred to it. A discussion on the modes of spelling applied by different writers and the probable differences of dialects precedes the linguistic part. We believe that the material for studying the phonetic laws of the Eskimo language is large enough to allow a more thorough investigation, and we consider the latter very desirable. Among the contents of the collection of stems, we have to call particular attention to the Greenland words occurring in traditions and in the sacred language of the priests. These words, as well as those which I collected among the central Eskimos, tend to show that many of the Alaskan stems which are lost in the common language still exist in the sacred language, and thus the most distant branches of the Eskimo stock are linked closer together. Besides, Rink has shown that a number of words that were considered exclusively western occur in certain derivations among the eastern tribes. Among these I mention the word *suk* ('man') of Alaska, which is found as *surosek* in the east. All recent researches tend to show that foreign influences upon the language are very slight, and the difference of dialect is probably entirely due to evolution.

The work of Dr. Rink will be highly appreciated by all ethnologists, and we have only to add the wish that the learned author will publish the originals of his large collection of Eskimo traditions, which would be highly welcome to students of American philology.

F. BOAS.

BOOK - REVIEWS.

The Children of Silence; or, The Story of the Deaf. By JOSEPH A. SEISS. Philadelphia, Porter & Coates. 8°.

THE object of this book is to excite interest in behalf of the deaf and dumb; and the means by which the author aims to do this is by presenting statistics of the numbers thus afflicted, the sad condition in which the deprivation leaves them, and an account of what has been done for their relief. Judged by the lenient standards which one must apply when considering it as a benevolent enterprise, the work is quite successfully done, and throughout urges the reader to a deeper knowledge of the subject than is here available. Regarded as a contribution to educational science, a less favorable

verdict must be passed. There is a lack of unity in the pages, and a much more serious lack of appreciation of the best literature (even that in English) on the subject. Books of this order have been frequently published, and have done much good in arousing the public to an intelligent interest in the lives of the defective classes. In 1835 Mr. John R. Burnett published at Newark, N.J., his 'Tales of the Deaf and Dumb,' which, though introducing much irrelevant matter, shows a deeper insight into the mental condition of the deaf-mute than the pages of Dr. Seiss; and, best of all, the 'Lost Senses,' by Kitto, contains a highly valuable description of the world, from the deaf man's point of view, by an eminent and observant scholar. Neither of these sources seems to have been utilized by the author. Again, in discussing intermarriage of near relatives as a cause of deafness, the author leaves the most interesting contribution to the subject (the memoir of Prof. Graham Bell) with a merest notice, though this is one of the topics to which he devotes most space.

The statistical element in the volume is as good as any thing we have. There are about thirty-five thousand deaf-mutes in the United States, but the defective method of taking the previous statistics makes it impossible to say whether deaf-mutism is on the decrease or not. The most probable average ratio of deaf-mutes to the population at large is 1 to 1,500, and this would give about a million of deaf-mutes in the world; and yet (in the United States at least, and probably elsewhere) the deaf form the smallest element of the defective classes, including under this term the blind, deaf, idiotic, and insane. Deafness, however, is a disease of childhood, and the number of deaf persons of school age is double that of the blind. It is interesting to add that there are about six deaf males to five deaf females, and that the notion that the deaf have an immunity from other diseases of the sense-organs is not borne out. Among the causes of deafness the intermarriage of near relatives is regarded as a very serious one. While some authors look upon such marriages as harmless when both parties are of a sound constitution, yet the bulk of the evidence is decidedly opposed to such unions, and finds in their offspring an undue proportion of nervous defects of all kinds. That the intermarriage of deaf-mutes is a fertile source of the increase of deaf-mutes is now generally admitted, and some regard one-third of all cases as due to this origin. A very large number of deaf-mutes are deaf from birth; and of those who become deaf, a very large percentage lose their hearing in the first, second, or third year of life. After this the liability to deafness rapidly decreases.

The relation between deafness and muteness is not a necessary one: it is because the ear educates the vocal mechanism that deaf persons become mute, not because their vocal organs are not correctly formed. This fact makes it possible to teach the deaf to vocalize; and the system by which they are taught to read the sounds on the lips of the speaker, while they answer by speaking as well as they can, is already the most widely adopted, and seems destined to supersede the finger-alphabet for general purposes. The question whether the blind or the deaf suffer the worse affliction has often been asked; and it is not generally known that on this point there is quite an agreement, among those most competent to judge, in favor of the blind. The deprivation of spoken language is in our civilization the most serious deficiency. The unsympathetic nature of the deaf as contrasted with the cheerfulness of the blind, as well as the fact that eminent blind persons are much more numerous than eminent deaf ones, speak for blindness as the less serious loss.

The history of the treatment of the deaf-mutes is an interesting one. Among savages they were generally considered as monsters, and put to death; for a long time they were held on a par with idiots; and the idea of their being educable was regarded as preposterous. When it is remembered that the first institute for their instruction was founded in 1765, and that the demonstration not long before of a deaf-mute's capacity to read was regarded as a miracle, one appreciates the truly modern mode of regarding them. It is not many years ago that they were first regarded as having the right of citizenship and other legal privileges.

While Dr. Seiss has thus put together in a shape likely to attract readers some useful information, he has left the field free for a really valuable and scientific treatise on deaf-mutism. Such a treatise

should contain a full account of the way in which they were regarded by different peoples, a history of the methods used to educate them, a psychological analysis of their state as illustrated by recent research, with special attention to their language, a good account of the physiology and pathology of deafness, and as much biographical matter as is really authentic. Such a general cyclopædia of deaf-mutism, and of blindness too, would be a great addition to the scientific appreciation of a most interesting portion of the human species.

NOTES AND NEWS.

THE Aristotelian Society of London has issued a very attractive programme of its winter work. The president, Mr. S. H. Hodgson, read a paper entitled 'The Unseen World' on Nov. 7, and the subsequent meetings are to be devoted to the following subjects: Nov. 21, 'The Psychological Laboratory at Leipzig,' Prof. J. M. Cattell; Dec. 5, 'Is Mind Synonymous with Consciousness?' the president, Messrs. S. Alexander, Bernard Bosanquet, D. G. Ritchie, and G. F. Stout; Dec. 19, 'Philosophy during the Period of the Renaissance,' Miss C. E. Plumptre; Jan. 9, 1888, 'Darwinism in Relation to Design,' G. J. Romanes; Jan. 23, 'The Philosophical Importance of a True Theory of Identity,' Bernard Bosanquet; Feb. 6, 'Wundt's Theory of Apperception,' J. S. Mann; Feb. 20, 'The Real Essence of Religion,' Rev. E. P. Scrymgeour; March 5, short papers on various subjects; March 19, 'Attention,' G. F. Stout; April 9, 'Heraclitus and his Philosophy,' Dr. Clair J. Grece; April 23, 'Conscience Theories,' Pasco Daphne; May 7, 'What is the Distinction between Desire and Will?' Professor Bain, W. R. Sorley, J. S. Mann; May 28, 'The Demarcations and Definitions of the Subject Sciences,' Professor Bain.

— The second number of the *Journal of Morphology* will appear about the first of January. The endeavor has been to produce, without counting expense or effort, a journal that will stand in the very first rank, and worthily represent its department of American science. The first number has been out long enough to be passed upon by the scientific public, and we think we may say without hesitation that the verdict has been as favorable as could possibly have been desired. Professor Mark of Harvard University writes, "The first number of the *Journal of Morphology*, so anxiously awaited by zoölogists, seems to me to surpass in every way the expectations even of those who have had the highest hopes for its success. Evidently no pains have been spared by either editor or publisher to make it first class. Every one in the country interested in zoölogy will be justly proud that at last we possess a zoölogical magazine which is equal to the best European publications." As a specimen of foreign opinion, we may quote from Prof. J. B. Carnoy of Louvain, Belgium: "This new review is splendid. I congratulate you sincerely on having treated science as it deserves." Of course, the expense of such a publication is very great, and the circulation necessarily limited; but it is, of course, very desirable that the journal should be self-supporting. All interested should at once send in their subscriptions to Ginn & Co.

— At a meeting of the Engineers' Club of St. Louis recently, Professor Nipher exhibited a steam-pipe 5 feet long and 6 inches in diameter, one end of which had been closed with a plain cast-iron cap. The cap of the pipe had been blown off bodily, without being broken up. The break extended around the cap just at the end of the pipe which had been screwed into it. He explained that the cap had been blown off by a ball from a Winchester rifle. This was done by standing the pipe vertically on its closed end, filling it with water, and firing vertically down upon the water. The floor upon which the pipe stood had yielded, and the cap of the pipe had been forced down so quickly that the pipe could not follow, so that the cap and pipe parted company. To force this cap off required a force of between 135,000 and 150,000 pounds, or about 70 tons. The ball was a 38-caliber ball, and the charge was the ordinary one of 40 grains of F. G. Dupont rifle-powder. The pipe weighed, exclusive of the cap, 96 pounds, and the cap weighed 9 pounds. The ball was greatly flattened by the water, but had not battered against the bottom of the pipe. Other experiments showed that the ball was stopped by the water by the time it had reached a depth of one foot.

languages of British Columbia. All this, it seems to me, argues in favor of the indigenous, American origin of the Eskimo.

A. F. CHAMBERLAIN.

University College, Toronto, Nov. 12.

IT seems to me that the similarities of sound mentioned in Mr. Chamberlain's letter cannot be admitted as evidence of a connection between the Eskimo and other American languages. The Eskimo words which he classes together are derivatives of entirely different stems, that cannot be traced to a common root. In the first table we recognize the following stems: *nipta*- ('clear weather'), *nipig*- ('to stick'), *nipag*- ('to vanish'). Under the heading *man* the words *inuk* and *angut* are classed together, although they have no connection whatever. In comparing languages, complicated derivatives must not be used, but the words must first be traced to their stems, and the meaning of the stems must be ascertained as well as the phonetic laws obtaining in the dialects of the stock, before it is possible to make a satisfactory comparison. Fortuitous coincidences of sound like those given by Chamberlain cannot be admitted as evidence of relationship.

F. BOAS.

New York, Nov. 25.

Rate of Change in American Languages.

THE letter of Dr. Beauchamp (*Science*, Nov. 18) opens an interesting linguistic question. My own impression is that the rapidity of changes in unwritten, at least American, languages has been overestimated.

Sagard, in the preface to his 'Dictionnaire de la Langue Huronne' (Paris, 1632), asserted that the Huron was constantly changing, so that in a generation or two it would seem like a new language. Two hundred years afterwards, Duponceau took Sagard's very imperfect book, tried it on some intelligent Hurons, and found that "the language had not undergone any essential change" (*Mémoire sur les Langues de l'Amérique du Nord*, pp. 444, 445).

In 1578 Jean de Lery printed his 'Histoire d'un Voyage fait en la terre du Bresil,' containing long conversations in Tupi. Three hundred years later, Dr. Nogueira republished these conversations, with their equivalents in modern Tupi. The differences are surprisingly small,—with proper allowances for dialect and varying phonetics, scarcely more than between Lery's French and the French of to-day (see NOGUEIRA, *Apontamentos sobre o Abaíenga ou Lingua Geral dos Brasis, Rio de Janeiro*, 1876).

I have recently completed a comparison between the Alagüilac of Guatemala, which is the most southern dialect known of the Nahuatl, by means of a vocabulary obtained in 1878, with that tongue as spoken in the valley of Mexico in 1550, preserved in the 'Vocabulario' of Molina. The separation of the two peoples could not have been less than four hundred years; but the divergencies are so slight that I could easily have believed the Alagüilac words to have been obtained by a German (my informant was of that nationality) in ancient Tezcuco.

Dr. Beauchamp, in referring to conflicting orthographies of the same word, points out a real but not the only cause of apparent without actual change in these tongues. He also touches on the confusion liable to occur from the natives forming diverse figurative compounds to express objects and ideas new to them. I was struck with this lately in comparing the expressions in the Lenâpé for 'faith,' 'regeneration,' 'repentance,' and such theological terms, as introduced, on the one hand, by the Moravian missionaries, and, on the other, independently, by the Anglican Church. They are usually totally dissimilar.

But a much more curious and important law underlies the apparent variability of many American tongues. I refer to that of 'alternating consonants' and 'permutable vowels.' In a number of these languages it is entirely optional with the speaker to articulate any one of three or four consonantal sounds for the same phonetic element. For example: he may at will pronounce the syllable *ton* either thus, or *lon*, *nol*, *rot*, etc., alternating the elements *l*, *n*, *r*, *t*, at will. I have little doubt but that something of the same kind obtained in ancient Accadian, which will explain why the same cuneiform character stands indiscriminately for the sounds *ku*, *tus*, *pun*, and *dur*; and the recent researches of Dr. Carl Abel on the phonetic modifications of the ancient Coptic radicals hint strongly at the prevalence of this peculiarity in that venerable speech.

In America, I name as special examples of this the Klamath and the Chapanec. But that these phonetic variations are within fixed limits, and do not involve the integrity of the language, is curiously proved by the last mentioned. Remesal, the early ecclesiastical historian of Chiapas, states that the Chapanec was introduced into that department from Nicaragua many generations before the Conquest; probably it was not later than the year 1300. Now, in 1872, my late friend, Dr. C. H. Berendt, collected in Nicaragua, from a few old Indians, the only survivors of their tribe who spoke its tongue, a number of words and phrases of a dialect called the 'Mangue.' A comparison proves it to have been beyond question a very close relative of the Chapanec, essentially the same in fact, though separated from it for more than five hundred years (see an article on the Mangue by me in the *Proceedings of the American Philosophical Society*, 1885).

As in the Turanian tongues, the Turkish, for example, there is a 'vocalic echo,' the leading vowel of the word forcing the others to assimilate to it in sound, so in some American tongues there is a 'consonantal echo,' the presence of one consonantal sound requiring more or less changes in the others. The Tupi, the Chapanec, and the Klamath offer examples of the 'consonantal echo,' while a certain degree of the 'vocalic echo' is observable in the Kiche and Cakchiquel.

These phonetic laws must be thoroughly understood and allowed for, before any one pronounces positively on the rate of change in American languages.

DR. D. G. BRINTON.

Media, Penn., Nov. 23.

Amnesia.

THE cases cited in *Science* (Nov. 11, 18, pp. 232, 250) remind me of the following. Some twenty-seven years ago, a neighbor of mine (a young man of twenty-five or under), springing from the vaulting-horse in a gymnasium to catch the trapeze, fell, striking apparently upon his shoulders, and was taken up insensible. Consciousness soon returned, perhaps in a fraction of an hour, but there was no recollection of the few hours just previous to the fall. As recovery progressed, however, it was said that his recollections came down closer and closer to the time of the accident; and that in a week or less he could even remember taking the leap, though not his striking the mattress.

Whether it be common that the progress of recovery should thus lessen the period covered by the amnesia, might no doubt be learned from such data as many professional athletes could furnish. An athlete once told me how, some years before, he had fallen on his forehead in the circus, and had been taken up for dead. His recovery, I think, had taken several months. He could remember, not indeed the blow, but the sense of powerlessness with which, in mid-air, he had realized that "his balance was lost." But perhaps he did not say whether, a few hours or weeks after the accident, his recollections had come down so far.

J. E. OLIVER.

Cornell University, Nov. 18.

THE cases of amnesia mentioned in *Science* of Nov. 18 recall in my own experience cases which may be of sufficient interest to be recorded.

When about fifteen years old, I went into a stable to stanchion cows for milking. About an hour afterwards I was found wandering about the yard unconscious, and bleeding profusely from wounds in the face. I have not been able to this day to tell how I was hurt. I have no recollection, beyond going into the stable and fastening a few cows. My hat was found under the cattle's feet. My front teeth were loosened, a hole cut through my lip, and my shoulder in front badly bruised. I was feeling well at the time, and have never fainted, and cannot refer the injury to that cause. The nature of the injury would indicate that it came from the front, and must have appealed to my senses in their normal state.

From other experiences I have always believed that it is more common to remember the cause of an injury producing temporary unconsciousness than to forget it. I became unconscious once from drowning, but remembered vividly every thing when restored. I was once prostrated by lightning, but remember having seen the flash.

I think one's remembering the cause of an injury depends largely

on whether it appeals strongly and for some time to the senses, especially of sight.

I know, from personal experience and observation, that it is not uncommon for man to temporarily lose the power of the senses under excitement, while the body still performs its normal functions intelligently. Perhaps this would explain the third case mentioned by Mr. Hall. The gentleman remembered the runaway, but became so excited in checking the horse that his senses were oblivious to all surroundings.

In my own case the cause evidently acted before my eyes, and I have been led to believe that the cause of an injury may act so suddenly as to produce unconsciousness before the impression made on the senses can reach the brain.

Another case was unconsciousness produced by poisoning with sulphuretted hydrogen. I went into the attic to regulate a generator, and shut the trap-door, as I had to pass over it to reach the generator. There was but one window in the room. It was down, and about fifteen feet away. There being no gas at the hoods in the laboratory led me to think the iron sulphide was out. I disconnected the tubing, and found high pressure, which forced several gallons of gas into my face. It produced involuntary respiration, and my lungs were drawn full. Deeply impressed that my only hope of life was fresh air, I started for the window at once. Almost instantly I began to get dizzy, and my vision was strongly impaired. The window, only a few feet away, seemed very remote, and no larger than my hand. My rapid advance toward it gave me the strange impression that my legs were half a mile long. I became unconscious before reaching the window, and all is a blank until I found myself rushing down the stairs, two stories below, still impressed with the necessity of reaching pure air. In an unconscious state, I raised the window about eight inches, raised the trap-door, and fell headlong down the stairs to the laboratory, and was found by one of the students deathly pale, the blood settled under my eyes, my muscles rigid, and large drops of cold perspiration on my face. Soon after the student reached me, I began to show signs of recovery, and suddenly sprang to my feet, exclaiming, "I must have air!" and rushed down the stairs, regaining consciousness on the way.

This case shows suspension of a train of thought which was taken up where it was left off, and pursued after a season of unconsciousness. It also shows several intelligent trains of thought pursued in the absence of general consciousness, leaving no impression on the memory. I have often asked myself what directed me to raise the window and the trap-door, and have wondered whether there are centres in the brain to direct intelligent action for self-preservation in the absence of consciousness.

My intention was to open the window for air, but I have no knowledge of having done it. This has led me to ask whether impressions made on the brain during consciousness may not be automatically executed after the avenues to the external world are closed. May not a state of partial unconsciousness, as in somnambulistic sleep, be produced by injury, and well-directed trains of thought be executed and leave no impression on the memory?

A friend of mine has a blank of three weeks in her life while sick with typhoid-fever, yet was unconscious only the last ten days of the time. I have always explained such cases on the basis that bodily condition has much to do with the indelibility of impressions made on the brain. When the body is weak, the impressions are weak and forgotten. Even in a state of health there are many perceptions that make no lasting impression. F. L. HARVEY.

Maine State College, Orono, Me., Nov. 22.

American Microscopes. — A Complaint.

EVERY autumn when the colleges and medical schools of the country begin their academic years, there are many students who come to their instructors seeking advice in regard to the purchase of microscopes. Often they appear already furnished with an instrument of which they are anxious to learn that the lenses are particularly good.

As it has been my duty for several years to conduct a large class in practical histology, I have had frequent applications for advice about microscopes, and have seen and examined a great many different stands, and the lenses of many manufacturers. I have had

therefore, opportunities to test the practical convenience and advantages of the many sorts of microscopes which the students have brought along with them. The result of this experience is the conviction that it is undesirable to recommend a student to purchase any microscope whatsoever of American manufacture, and to always counsel him to obtain, if possible, one of the German or French instruments.

In order to make my judgment more clear, I may add that I know of no American microscope which I should like to purchase at any price, for my own use in histological or embryological work.

I venture to express this adverse opinion in regard to American microscopes in the hope of inducing some of our opticians to manufacture a stand for a microscope suitable for the use of students of histology and biology. It appears to me that the simple model now almost universally adopted in Europe is far superior to every thing offered us in rivalry to them by our own dealers.

To justify myself, I should like to give, first, the reasons for my disapproval of the American forms; and, second, the reasons for my preference of German forms. The fundamental error in microscopes of American manufacture is that they are for the most part constructed with a view of, I might almost say, entrapping inexperienced purchasers. The zeal of the maker is turned too much to decorative lacquering and nickel-plating: he adds to his stands as great a variety of mechanical contrivances and adjustments as the price of the stand will permit, and many of these contrivances are not really commendable for their utility. In the majority of cases the stands are made to tilt, which, for one that uses the microscope for real work, is an almost useless luxury, because he who really works in histology necessarily examines fresh specimens in fluids, or at least constantly has on the stage of his microscope preparations in various stages of unreadiness, and not mounted in a permanent form. All this implies the constant use of fluids, and, if the stage of the microscope is inclined, the use of liquids is impracticable. Any one, therefore, who uses his microscope for the ordinary purposes of a student or an investigator, or in connection with clinical or pathological work, very soon falls out of the habit of tilting his microscope. Hence it is, that, while making a microscope to tilt renders it considerably more expensive, it adds nothing essential to the convenience of the stand for laboratory work. This same fact, that most of the work must be done with the tube of the microscope vertical, renders it indispensable that the microscope should not be too high; so that we must put down the ten-inch tube as a bad feature for a student's microscope. A rack and pinion is undoubtedly advantageous: it renders the use of the microscope more convenient, and increases its durability by diminishing the strain upon the stand during the coarse adjustment of the focus. When this adjustment is effected by shoving the tube with the hand, the microscope wears out sooner than with the rack-and-pinion movement; yet even the rack and pinion, which are so generally put on our American microscopes, are not indispensable, and the greater part of the histological and embryological investigations of the past twenty years have been made without the employment of this convenience.

The stage of the American microscope is very faulty. The large movable glass plate with a hole through it is a toy fit only for an amateur or fancy collector: it interferes with the use of fluids, and with the freedom of movement of the slide over the field of the microscope,—the two things which are most indispensable in practice. A good stage should be large and flat, with nothing upon it except a pair of spring clips and a hole for a diaphragm. The diaphragms are often a matter of particularly fanciful construction. Thus the Iris diaphragm is often introduced to allure the inexperienced, but it is not a good form except in conjunction with an acromatic condenser. There are other details of construction which are equally open to unfavorable criticism, but it is unnecessary to go into their discussion.

Unfortunately, while we see so much pains expended upon the brass-work of the microscope, we see a neglect of the optical members of the instrument; so that the microscope as a whole is converted into a showy piece of apparatus, and the eye-pieces and objectives are generally, though not always, of a decidedly inferior character: when they are really good, the lenses are very expensive.

If, now, our manufacturers would reverse the distribution of their

painstaking, and make a simple stand of small size and compact model with first-class lenses, they would furnish something which could be recommended to students and others by conscientious advisers.

Turning now to the consideration of continental microscopes, so universally used in Europe, and now happily gaining supremacy in this country, we see at once that they conform to the practical requirements which are disdained in the making of most American microscopes.

They are built with a firm base. The stage is easily reached by the fingers when the hand is resting upon the table. It carries no superfluous appurtenances, but is large and flat. The eye-piece is of such a height, that when the instrument is vertical it is easy to look into it. Concerning the lenses, it must be said that most of the European manufacturers are very conscientious in regard to those which they furnish. There are, of course, some makers who put upon the market objectives of inferior quality, and which are sold as such, and therefore at a correspondingly low price. This is of course legitimate, as there is a demand for cheap microscopes.

The price of these desirable microscopes is very much less than that of undesirable American ones. According to our system of protection, the physicians, scientific men, and students are taxed enormously if they buy a foreign instrument. Put into plain English, this means that we are heavily fined if we secure what we require in the way of microscopes, while a small number of manufacturers, whose money-making is of very little significance to the public, receive a bonus for furnishing an inferior article at a high price. Thus what is really important is sacrificed for what is unimportant. Many valuable members of the nation are sacrificed by being obliged to pay for the advantage of a small number of men who have never shown themselves willing to supply to those by whose sacrifices they benefit, the kind of instrument wanted.

Can any thing be more unjust? and are not we, who are engaged in university careers, in the practice of medicine, or any other useful occupation requiring the employment of microscopes, justified in complaining of the condition of affairs, which is little short of a national calamity? Is it unreasonable to ask the manufacturers of microscopes in this country to furnish us instruments of the kind we really need, as some sort of acknowledgment of the money they extract from us whether we will or not?

In expressing myself so decisively and emphatically upon the subject of American microscopes, I have not considered it necessary to give a detailed discussion of the relative merits and demerits of the different makes, because what I have expressed is the opinion, in these matters, of all the competent judges with whom I have talked on the subject.

I know positively that many of the best scientific men of America are ready to join me in saying, as I said at the beginning, that there is no American microscope which we should like to buy at any price for our own use.

CHARLES SEDGWICK MINOT.

Boston, Nov. 24.

The 'Act of God' and 'Fuerza Mayor.'

MR. MORGAN'S article in *Science* of Nov. 18, 'The "Act of God" and the Railway-Company,' is highly interesting, and suggests an illustration drawn from comparative national jurisprudence. The English common-law doctrine of the 'Act of God' appears very scientifically elaborated in the laws of Mexico under the title of 'fuerza mayor.' Our neighboring republic is greatly advanced in the science of law. While certain disturbing elements there interfere somewhat with the practical application of statutes at times, according to our views at least, nowhere on the continent has the science of law been more carefully studied, and the results of that study more accurately defined and set forth in both constitutional and statutory form. Religious faith, too, in Mexico is to-day as living and active a force in common personal life with the great body of the people as it was in Europe in the middle ages; and this fact again, as Mr. Morgan's article suggests, illustrates how, while the limitations of the Old-World doctrine have been gradually narrowing in the United States, it still holds its ground in Mexico with proportions which practically make it the leading condition of all contracts, expressed and implied.

As an instance of how this provision enters into express contracts,

let us take the great railway-concessions to the leading American companies. In these concessions 'fuerza mayor' generally appears in three distinct places. The obligations of the company to build within certain fixed periods are suspended in case of 'fuerza mayor.' The concessions are forfeited by the companies carrying any foreign armed force or goods contraband of war, unless they can show that this was done because they were unable to resist 'fuerza mayor.' Certain bounties granted to the railways cease during the time that the operation of the lines is suspended, even if the suspension should take place by reason of 'fuerza mayor.'

In the smaller transactions of daily life this doctrine continually appears as an unwritten law, which suspends all other laws, or contracts, or obligations. Superior force, which often in Mexico means what would simply be called disaster in the United States, is to the Mexican mind a good defence against almost any obligation. For instance: should one lease a boat for a month at a fixed sum, and unusual storms prevent using the boat for half the month, that would be ample reason why the lessee should tender only half the rent to the lessor, and he feel constrained to accept the offer.

'Fuerza mayor' is translated as 'superior force,' or 'uncontrollable circumstances.' These circumstances are nowhere, to my knowledge, defined, but the facts of what are uncontrollable circumstances are to be decided in each case. The coercion of an armed force is 'fuerza mayor.' The violence of storms is 'fuerza mayor.' The flooding of a river is 'fuerza mayor.' And, as before remarked, very generally what we are apt to consider as disaster, in Mexico becomes 'fuerza mayor,' and operates to relieve a contract of its obligations. To the American mind a contract made must be carried out, and disaster, if there is any, falls on the man who has loosely guarded his contract. In the confluence of the American and Mexican civilizations now taking place, it becomes an interesting question how this wide difference between the usage and thinking of the two countries will adjust itself.

W. W. NEVIN.

New York, Nov. 21.

The American Physique.

IN order to find out how closely the figures of makers of knit goods would correspond to those of the clothiers, I sent a letter to one of the largest manufacturers. I enclose his reply, together with the figures. You will observe that the figures on men's ware correspond very closely with those of the clothiers, making allowance for the tighter fit of the undergarments.

EDWARD ATKINSON.

EDWARD ATKINSON, ESQ.

Dear Sir, — Your favor of the 11th inst. was duly received, but the article referred to was not enclosed. It is impossible to give a perfect assortment of sizes of underwear for men and women, as the assortment varies in the weight of goods, and the section of country they are for. I enclose, however, a copy of an average order for 1,040 dozen of men's shirts and drawers, and one for 507 dozen ladies' vests and drawers, which will show very closely the sizes that we sell, and the proportion of shirts and drawers:—

	26	28	30	32	34	36	38	40	42	44	46	48	50	52
Men's shirts.....					88	124	128	84	52	32	16	10	4	1
Men's drawers....		40	88	104	92	68	44	28	20	12	4	2		

	26	28	30	32	34	36	38	40	42	44	46	48	50	52
Ladies' vests	45	113	127½	82½	34½	15	7½	1½						
Ladies' Drawers.....	4½	15	22	18	12	6	3							

Queries.

18. METEOR-FALL. — A few days ago there appeared in the newspapers a circumstantial account of the fall of a two-ton meteorite in front of a bank in the town of Amsterdam, N.Y. I have seen nothing but this first announcement about it, and fear the whole story may be a canard, yet would like to know that it was a genuine happening. Can you report the matter in *Science*, and doubtless oblige many others besides?

C. H. AMES.

Boston, Nov. 27.